information is obtainable, and it is to the performance of such a duty I now invite the members of this Society.

The manners and customs of the Aborigines, would necessarily be an accompaniment to any inquiry into their dialects, but with this difference, that much greater circumspection will be requisite to eliminate truth from fiction—actual well-established usage and faith (if any) from artifice and invention.

To the above general sketch of the information required, I may add, that the inquiries instituted will, in all likelihood, render possible the compilation of a map, showing the distribution of the various tribes, and may also throw some additional light on the peculiar productions of some of the less-known regions of the country.

For the accomplishment of the above objects I beg to propose the formation of Section E, for Geographical and Ethnological Science, under Law 60, and that Professor Halford, Mr. Gideon Lang, Mr. J. J. Stutzer, Mr. James Bonwick, and myself, be the first members, and that Professor Halford be the chairman of the section.

ART. VI.—*Probable Astronomical Causes of the Contortions in Palæozoic Strata, and the Prevailing Meridianal Strike of the Older Rocks of Victoria.* By THOMAS HARRISON.

[Read 16th October, 1865.]

At the very commencement of this paper, I would especially wish to disclaim aught like a desire to set up a theory. At the utmost I simply ask leave to read before your Society certain notes and observations which have occupied my attention for the past three or four years. I do even this interrogatively. I lay the various facts and reasonings before you as a humble seeker after truth, and not as one by whom truth has already been discovered.

To my mind there seems to be some show of reason in what will be advanced. That, however, will be for your Society to determine. Should I be so fortunate as to have, almost inadvertently, hit upon the solution of a geological difficulty, I am well assured all present will rejoice equally with myself. Or if, on the other hand, I have been pursuing a mere shadow, the sooner my eyes are open to the error, the

better will it be both for myself and all persons concerned. I assure you that when the thought first struck me I was just as much inclined to laugh at it as absurdity as will be the most sceptical of yourselves. It was only after facts had forced themselves upon my notice, time after time, that I mustered up sufficient courage to resolve upon my present step.

I was first led to the conclusion that other than mere local upheavals had determined the dip, strike, and contortion of the palæozoic strata, by a consideration of the rocks near Melbourne. I find that there, and indeed over the whole colony, the strike is nearly always meridianal, and the contortions of such a nature that they could not possibly have been caused by simply upheaving forces; a lateral pressure seeming to be absolutely required.

This lateral pressure, so universally demanded to explain geological phenomena, is not accounted for by any theories of cosmical action that I am aware of. Sir Henry De la Beche certainly suggests how it might be originated by the beds of strata becoming oxydized, thus occupying more space than in their pristine condition. He, however, appears to have forgotten that the same deposits must have been already in the form of oxides when first laid down.

But even this theory of a general expansion of the crust, together with that which supposes the interior of the earth to have contracted as the central heat diminished, fail to account for the singularly regular, even mathematically straight, direction of the general strike of Australian palæozoic strata.

Let us suppose the substance of an artificial globe to shrink independently of the paper covering, or an apple to shrivel in the interior while the rind remained of its original size. Wrinkles would occur in all directions, but such wrinkles would have but little regularity of arrangement, and aught like a parallellism of ridges or of depressions, extending over any wide space, could not possibly be looked for.

I know of no other theory so well accounting for the phenomenon—supposing it to be by any means general, and with few exceptions it seems, as regards palæozoic deposits, to be manifested over large tracts of the earth's surface—as an absolute change in the figure of the earth itself.

That the earth is spheroidal, and not spherical, we know from astronomical observations made with the greatest exactness. We know, too, that the spheroidity is just, or

nearly what it ought to be, by the theory of gravitating and centrifugal forces. If, then, the spheroidity is just what it should be from the earth's present rate of axial revolution, any greater rate of revolution in the past must necessarily have resulted in a less spherical form than that now obtaining.

I. Let us see what would be the result of any change in form such as the one suggested; that is, from a spheroid, the difference of whose diameters was excessive, to one in which the two diameters approximated to each other.

First. The equatorial diameter and circumference would be shortened, and as the dimensions of the crust or solid covering of the molten nucleus within would remain constant, a puckering in such crust would be the natural sequence. The force, also, causing these puckers, would be in the direction of the parallels of latitude, so that the corrugations (if I may so term them) would run in a strictly meridianal direction. This phenomenon of the puckers would be common over all tropical and semi-tropical regions, save in the exceptional cases to be presently mentioned.

Secondly. The polar diameter would be increased. Hence, at the arctic and antarctic circles, the molten matter within would tend to swell outwards, and as the same would act upon a rigid crust, such crust would be fractured in a stellar form, the rays proceeding from each pole as a centre.

Thirdly. Between these two regions of contrary disturbances would be one of quiescence. This region would correspond, or nearly so, with the temperate zone.

II. It is interesting to see whether marks of these supposed disturbances are anywhere apparent in the arrangement of strata, and other physical features of the earth's surface.

First. In Victoria, as was previously stated, the strike of the strata is, generally, nearly north and south. This strike seems to prevail over very large portions of the Continent.

Secondly. In a paper by Mr. Searles Wood, appearing in the *Philosophical Magazine*, this peculiarity of strike is stated to be incidental to palæozoic strata nearly all over the world; that is where such strata are at all contorted.

Thirdly. The principal mountain systems in which palæozoic rocks prevail run nearly north and south, as witness the Andes, the Rocky Mountains, the Ural, and the Australian Alps. Near the equator in Brazil, too, are numerous chains of mountains, all of which, by Johnson's

Map of the World, appear to have a meridianial direction. I see by the map in Murchison's Siluria that Silurian strata abound in that part of the Continent.

Fourthly. The trend of the land is generally north and south. This fact has been often noticed, but never satisfactorily accounted for.

Fifthly. The Andes diminish in altitude as they approach the south, as though the lateral pressure was there, as it were, dying out through an approach to the region of quiescence before alluded to. In New Zealand, lying much farther south than ourselves, the palæozoic strata are less distorted than in Victoria. In Russia, also, situate in the supposed northern region of quiescence, the Silurian strata are often, as I believe, found in a nearly horizontal position.

Sixthly. The northern and southern polar circles are either covered by drift or by ocean, so that the supposed stellar fractures are not observable.

III. It will be asked whether any force capable of diminishing the rate of the earth's axial revolution exists in nature.

1. Up to a very recent period the rate of the earth's diurnal revolution has been spoken of as constant. This conclusion, however, is doubted by eminent astronomers of the present day. *There is a retarding force.* The same being found in the resistance to the earth's axial revolution, resulting from the friction of the tidal wave, which, lagging a little behind the moon, impinges upon the shores of continents and islands, so that force becomes changed into heat, which is lost completely by being radiated into space.

The consequences of this force are apparent, in some few discrepancies discovered of late in certain astronomical tables.

2. There appears to me to be yet another source of retardation. Two deep water currents are continually running from the several poles to the equator. As these currents respectively pass from points whereat the axial velocity is nothing, to where it attains nearly one thousand miles per hour, the waters are left a little behind the earth, so that the currents really take a N.W. and S.W. direction, and, by impinging upon the easterly coasts of continental masses, must necessarily constitute a resisting power. There is, I know, a counter current, wherein the conditions are reversed, running from the equator to the poles; but from the evaporation in low being in excess of that in

high latitudes, the two currents are by no means equal in amount, so that the balance of force would go to retard the earth's motion.

These forces are, it must be admitted, most ridiculously small, but even an almost infinitesimal force, acting continually, must inevitably stop a motion, however swift, and put an end to a momentum, however great. There is no resource, therefore, but to conclude that these respective agencies—if you admit them—acting throughout countless ages, have tended, and are materially tending, to bring about an utter stoppage of our planet's axial revolution.

IV. There are some few astronomical facts relative to other planets of our system which may serve to throw still further light upon this subject.

The moon, which has no axial revolution, is quite spherical. Seen through a telescope, too, it presents a feature —for my own part I attach but little importance to the fact —yet, as bearing upon the present subject, the feature is rather remarkable. Near the satellite's upper limb is the great crater of Tycho, and from this as a centre run a number of radiating lines, corresponding with (supposing this crater to have been once the moon's pole, when she had an axial revolution like the other planets) the hypothetical star-like fissures at the north and south poles of this our earth. Adjacent to the space over which the rays extend is a region seemingly of quiescence, and beyond is what seems to me a zone in which a puckering up of the surface has taken place, analogous to that supposed in the tropical regions of our own globe. I lay but little stress upon this fact, and merely mention it in passing and by the way.

The surface of no other planet is thus open to telescopic inspection, but there are certain physical facts connected with several illustrative of the theory in a measure somewhat remarkable.

In the first place there is a very striking difference in the rate of their respective rates of axial revolution. Nearly all revolve in different times, and there is this peculiarity, that the slowest motions are just where the quickest ought to be expected, and *vice versa.*

Thus the Earth's period of axial revolution is twenty-four hours, but that of Jupiter, a much larger planet, is only twelve. Saturn, less than Jupiter, but an immense body as compared with the earth, is ten and a half hours. That of Mars (a planet whose diameter is half that of our own) is

twenty-four hours. The respective velocities and diameters of Venus and the Earth nearly correspond.

Now granting, for the sake of argument, what is by no means improbable, that all the planets had the same initial rate of axial velocity, and granting also that currental influences were in each, continually tending to bring about a stoppage, what was likely to happen would be nearly as follows :

Jupiter and Saturn being large bodies, would, notwithstanding these retarding forces, maintain their respective velocities for a lengthened period, just as a heavy fly-wheel acts as a more efficient reservoir of power, and runs longer, from the same impulse, than a light one. The motion of the earth as a small sphere would be speedily slackened, whilst the satellites (they being of diminutive size, and, moreover, subject to an inordinate amount of attraction from their respective planets) would soon come to a standstill, their axial revolution ceasing altogether.

Venus and Mars would seem to present an anomaly, since, having no satellites, they could have no lunar tides. The apparent anomaly, however, diminishes as examination proceeds. Venus would have a considerable solar tide, whilst the currents running from her poles to her equator must, from the intense heat to which the planet is subjected, be greatly in excess of those acting upon the earth. Mars, from its small size, might also have a considerable solar tide. There is, however, a difficulty with respect to this planet which I am utterly unable to explain away.

What, however, would go far to show that the axial velocities of the several planets are not constant, is the discrepancies manifest in the respective spheroidities of the bodies as compared with the rate of their diurnal revolutions. Taking the spheroidity of Jupiter to be what it ought to be from the tremendous velocity with which that orb revolves, that of Mars would be greatly in excess of the amount assignable to it by calculation. To my mind the cause of this lies in the fact that Jupiter, as a large body, and retaining its heat longer, is not yet completely solidified, or has only a proportionately thin crust, and is, therefore, sufficiently plastic to allow of gravitating forces acting at once, and as soon as they in any way preponderate over the centrifugal force, giving the planet spheroidity. Mars, however, being of much smaller dimensions, would soon lose its heat, and as soon grow rigid. From this cause the latter planet might

retain its pristine form long after the spheroidity of that form was greatly in excess of what it should be from the rate of axial revolution.

Venus, on the other hand, may have retained a large amount of original heat, not because of the vastness of her mass, but on account of radiation being checked by proximity to the Sun. She may, therefore, remain nearly as plastic from this cause as in the far off Jupiter from quite a different one. Hence she is even more spherical than the earth. If this hypothesis, however, be true, then the earth is even now somewhat more spheroidal than it should be—surmises as to this being the case have, I think, ere this engaged the attention of astronomers.

Granting that this theory be true, it will readily be seen how the changes spoken of are likely, not only to cause contortion of strata, but also to act upon and alter the mean temperature of the globe. It is pretty generally acknowledged that the average climate of a particular region is not wholly dependant upon the region's latitude. Islands enjoy a more equable temperature than does land thrown into large masses. It is inferred, therefore, that the relative amounts of heat, received at given times, from the sun may be identically the same, yet that two or more very different climates may prevail. Sir Charles Lyell's theory of climates is well known. "With all the land gathered round the poles," he argues, "we should experience a maximum of cold; with a preponderance of land at the equator, the reverse conditions would prevail." Geologically this theory is of great practical importance. Very many diverse climates have, at different times, prevailed upon the earth. Taking from the close of the Cretaceous age, we find that at the dawn of the Tertiary epoch tropical fruits grew in what is now England, and that a tropical fauna prevailed whilst our own Victorian Eocene beds were laid down. At a much later period England appears to have been submerged beneath an arctic sea; its mountains covered with glaciers, and its ocean dotted with icebergs. A little attention will show how these facts bear upon the hypothesis under consideration. As the rate of the earth's axial revolution diminished, the central molten nucleus would be the first portion losing its spheroidity. The rigid crust might, and would, retain its former shape for a lengthened period. There would thus be a hollow beneath the equator, and an increased pressure tending to cause disruptions at either pole; but for a time this state of

instability would continue. Meanwhile the waters would arrange themselves, not parallel with the spheroidal crust, but concentrically with the spherical mass within. Two large oceans would thus accumulate near the poles, thus uncovering the major part of the equatorial regions. Under this state of things the earth's climate would increase in temperature. Then, at last, as convulsions shattered the crust, or as the simple force of gravitation caused the arch at the equator to give way, and the pressure at the poles overcame the resistance confining the molten matter within, would the equilibrium be restored, water would again occupy the torrid zone, and some portion at least of the polar oceans would be left dry ground.

I do not, however, think that the restoration of equilibrium and exposure of the polar continents were the real causes of the intense cold incidental to the glacial epoch. The hypothetical hardly meet the real facts of the era, as made known by observation. On the other hand, high northern and southern latitudes appear to have been even more submerged than at present. A glacial climate might result from conditions somewhat the reverse of those described. That a vast tract of land in equatorial regions should cause a tropical climate to prevail in high latitudes it is necessary that such land should be of a low, or at least of a moderately low, level. Supposing an excess of land of great altitude, such as the land of the plateau of Mexico or of Thibet, to be gathered round the equatorial regions, a very different state of things would result. The equatorial regions might then be covered by perpetual snow from excess of altitude, just as are the plains of Nova Zembla by height of latitude. In such a case the southernly breezes of Europe would be as chilly as those coming from the north, simply that both would pass over equally frozen tracts of land. And this mass of high table land might very naturally be looked for under the conditions supposed.

In the midst of the arctic climate so caused we may imagine that the equilibrium is somewhat suddenly restored. Tremendous currents would rush over what are now temperate regions; ample cause, it would seem, to account for the glacial drift of both northern and southern hemispheres, and for a very extensive destruction of animal and vegetable forms. Could this be the moment of which the second verse of Genesis speaks: "*When* the earth was without form and void, and darkness was upon the face of the deep, and the

Spirit of God moved upon the face of the waters?" And can it be that, when "the fountains of the great deep were broken up" at the succeeding flood of Noah, the yet unstable crust of the earth sunk still further, thus, by suddenly changing the centre of gravity, inducing another rush of waters deluging the world?

It will be objected that, if the lessening of velocity necessary to produce the phenomena spoken of was ever manifested in the past, the same ought to be observable at the present time. Astronomical observations are now carried on with so much of precision that an almost infinitesimal increase or decrease in the rate of the earth's diurnal motion would soon be detected. An analogous objection was once urged against the Copernican system. It was said that if the earth really revolved around the Sun in an orbit many millions of miles in diameter, the difference of place on the part of the observer must, at two different periods of the year, give rise to a great apparent displacement of the fixed stars. The absence of parallax is, however, easily accounted for, taking into consideration the vast distances of the observed bodies. If, in astronomy, Sirius is so far removed that 180 millions of miles, the diameter of the earth's orbit, dwindles down to a point when viewed from that distant star, may not geology deal with times so inconceivably extended that any diminution in the earth's motion is not to be discovered, although sought after during observations extending over many centuries.

It will be objected also that the amount of contraction required to produce effects such as are witnessed in Victoria, for example, would be so vast that no diminution in the earth's rate of revolution would be likely to give rise to it. It will not, however, follow that this crumpling of beds extends completely round the earth in any one zone. Crumpling of the crust could only take place where the substratum was somewhat of a yielding nature. Take an arch composed of alternate blocks of granite and soft brick, and subject this arch to enormous pressure; it will be the bricks that will give way, whilst the blocks of granite will be totally unaffected by the forces brought to bear. If, over the outside of the whole arch there be pasted layers of paper, crumpling of these will occur on the compressible brick portions only, and will remain undisturbed above the harder granite. It may happen, therefore, that it is only in the continental and island masses where such disturbances

have taken place, and below the ocean strata may still lie horizontal as when first laid down.

It will be objected further, that the strike of palæozoic strata is not always meridianal. This, I think, is no more than might naturally be expected. The contortions of strata could only take place over such portions of the substratum as gave way under the gravitating pressure. If any of these yielding areas happened to run in a zig zag direction, like the course of the Andes, for example, the strike of the superincumbent rocks would be materially influenced. And, again, local disturbances, such as might be caused by volcanic action, may have materially tended to give certain local directions to the strike.

It must not be lost sight of, either, that if the earth is really formed of a thin crust covering a still molten centre, and if even this crust at one time existed in a molten state, there is no possible alternative but to suppose that, as the heated mass gradually lost heat, the whole would greatly contract. The result of such contraction would be somewhat to diminish the tendency to bulge out at the poles, to increase the puckers at the equator, to remove the regions of quiescence to higher and higher northern and southern latitudes, and, perhaps, a little to increase the complication of the various puckerings seen upon the earth's surface.

This idea of the earth contracting by loss of heat is by no means new, the only difficulty being an astronomical one, since any diminution in the earth's volume would result in an increased rate of diurnal revolution, and no such increase is to be detected by the nicest observations. But may it not happen that, just at the present moment, these two results balance each other? May not the acceleration consequent upon contraction be, at this time, exactly counteracted by the retardation arising from friction of the tidal wave and currental influence?

It will, perhaps, be objected, that the time requisite to bring about the observed changes in this manner would be immensely beyond the limits of all reasonable hypothesis. I am, however, not quite sure of that. Take the whole series of rocks, and, in imagination, run over the list, from the granite upwards to the last bed of tertiary deposits. In Victoria alone Mr. Selwyn calculates the Silurian rocks to be upwards of 35,000 feet (nearly seven miles) in thickness, and this is only a fraction of the great thickness of strata manifested in other parts of the world. Remember that a

vast proportion of this thickness was evidently deposited in the deep sea, where accumulation could only go on with extreme slowness; and remember, too, how long a time would be required to silt up our own Hobson's Bay, close to the shore, and of very inconsiderable depth. Or take another geological phenomenon, that of denudation. How long must it have taken for the most rapid currents, and the most boisterous seas, to have cut down and carried away that vast amount of strata which has evidently been removed all over the Australian continent. Not a few geologists have gone so far as to suggest whether the erosion of gorges and valleys in granitic mountain chains has not been the result of long continued atmospheric action alone. Grant this, and surely the objection raised upon the score of time must be abandoned.

I must again request that you will not look upon the present paper as an attempt to set up a theory. So far as I have had an opportunity of examining the rocks of this colony, they certainly, to my mind, appear to favour the suppositions herein advanced. It may happen, however, that observations made in other lands might have led me to very different conclusions. It is in order that I may supplement my own facts and observations with the facts and observations of other and more experienced geologists that, with the greatest diffidence, I lay this paper before your Society.

ART. VII.—*A Report on the Results of an Exhibition of Gems, both Victorian and Foreign, and Works in the Jewellers' Art, in both Gold and Silver, held in the Hall of the Royal Society of Victoria during the week ending the 6th May,* 1865. Drawn and presented to the Royal Society by the REV. J. J. BLEASDALE, D.D., the President for the current year, 1865.

TO THE COUNCIL AND THE MEMBERS OF THE ROYAL SOCIETY.

Gentlemen,—I have the honour to submit the following Report on the Exhibition of Gems and Jewellery, which originated in a suggestion of mine made some months ago in a short paper read before this Society, and which, under your auspices, has now been brought to a successful termination.